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THE
OPHTHALMIC RECORD.

VOL. IV. DECEMBER, 1894. No. 6.

CRITICISM CONCERNING RECENT VIEWS AS TO THE SECRETORY FUNCTION OF THE CILIARY BODY.*

By JOHN GRIFFITH, F.R.C.S.
Pathologist to the Royal Westminster Ophthalmic Hospital, London.

Mr. President and Gentlemen:

A critical review of this subject from its infancy would entail endless labor, necessitate a long rigmarole of different views and outcomes of experimental study, and, in the end, could scarcely be anything else than a feeble, distorted shadow of the masterly writings of Dr. W. Nicati, of Marseilles.

[1]Nicati has, in this recent work of his on the origin and destination of the aqueous humour, made an exhaustive collection of literature pertaining to it, entered fully into the anatomy and physiology of the ciliary region, besides giving a description of his own experimental researches, and the conclusion at which he has arrived. So recent and thorough a revision would leave little to write about, but for the opening up of a wider question by the discovery, almost at the same time as Nicati was elucidating this problem in France, of ciliary glands, by [2]Mr. Treacher Collins in this country.

*Read in Section of Ophthalmology, British Medical Association, at Bristol, 1894.

[1]La glande of l'humeur aqueuse. Archives d'ophtalmologie, 1890, p. 481.

[2]The glands of the ciliary body in the human eye. Trans. Ophth. Soc. Vol. xi. p. 55.

Collins, who has made a special study of the histology of the eye, attributes the formation of the aqueous humour to these glands.

Many English ophthalmologists have accepted his theory without dissent; others still cling to the older views that the ciliary processes form the fountain-head of this intra-ocular secretion.

Nicati terms the pars ciliaris retina, together with the subjacent pigmentary epithelium, "the gland of the aqueous humour, the gland of the ciliary processes, or the uveal gland," and considers it the elaborating organ through which the blood-serum derived from the chorio-capillaris passes in its preparation for aqueous fluid.

It has been proved sufficiently by the many experiments of Deutschmann, Ehrlich, Schocler and Uhthoff, Leplat, Memorsky, Leber, and others, that the ciliary region is responsible for this fluid, and so any discussion on that point would be superfluous. A preliminary account of the histology of the ciliary region must obviously be necessary to place this subject on a sound footing.

Upon looking at the minor aspect of the anterior half of a bisected eye from which the vitreous and lens have been removed, two zones are visible between the ora serrata and the base of the iris, a posterior, wider and darker zone without any distinct elevation—the pars non-plicata, and an anterior, narrower, and lighter portion, which is thrown into numerous meridional ridges—the pars plicata.

The pars plicata is an apparently complex structure, for it has given rise to many misshapen views and ideas, especially to microscopists; yet a correct knowledge of its anatomy, described by [1]Brailey, simplifies it considerably. Under the microscope these ciliary processes or ridges appear to be broken

[1]Some points in the anatomy of the Ciliary Body. British Med. Journal, Vol. II. 1882, p. 577.

up into foliaceous papillæ, the anterior of which are often seen folded backwards. Such is not really the case; it is a histological deception, and is a consequence of their strange, undulating outline. It is, as one would expect, difficult, and I might say impossible, to get a perfect antero-posterior section of any single ridge. This undulating appearance is readily seen and understood in sections cut through a plane at right angles to their perpendicular; also in coronal sections.

The acme of each ridge is in front, their height being a little less than 1 mm., their length about 1.5 mm.

Around the circumference of the lens, there are about seventy in number, and alternately, in the valleys between, are smaller ridges. Sometimes two of the latter are seen which coalesce anteriorly either by the gradual rising up of the floor between them, or, as pointed out by Brailey, by the fusion of their summits, which gives rise to short cæcal pits directed forward. The appearance produced by the undulations of the approximate sides of adjacent large ridges with the folded arrangement of the intervening embryonic ones, is very striking, and has the resemblance of a racemose gland. The reduplications are very extensive, and must increase the surface area of the anterior plicated zone considerably.

Upon microscopic examination, we find that the retina, not taking into account its pigmented epithelium, ceases at the ora serrata 5.5 mm. behind the sclero-corneal junction. Here its structural elements resolve themselves into a single layer of elongated cells, having a decided inclination forward. This can be traced as a non-pigmented membrane as far as the base of the iris, and constitutes the pars ciliaris retina.

In the hinder part of the non-plicated portion of the ciliary body, these cells are elongated and fusiform, subsiding gradually into columnar, and lastly cubical-shaped, elements, as they reach forward to the ciliary processes, around which they also retain this cubical form. From some of them, which no

doubt are epiblastic and the representative of the retina, cords arise which, blending with the outer layer of the split hyaloid membrane, constitute the main structure of the suspensory ligament of the lens, and are inserted into the anterior aspect of the lens capsule near its equator. The structural peculiarities of the pigmentary epithelium in this region, the continuation forward of the tapetum nigrum of the retina, demand the fullest investigation. This can be done only by studying bleached sections, and to Collins is due the credit of having first attempted this with success. He points out the difficulty of his method in the following words:

"The manipulation of the sections requires considerable care, as, after exposure some time to Chlorine, they have a great tendency to come to pieces." He then describes how they fall away, and adds: "It is not easy to get a perfect section first bleached and then stained."

I fully endorse Collins' statement, having on several occasions attempted his method without success. The plan I have adopted is to my mind decidedly superior, as the chief difficulty is overcome, viz.: the brittleness of the specimen. The tissue is bleached *en bloc*; it cuts without difficulty, and the sections, not in the least brittle, will stain with eosin. In fact there is no trouble at all in the process if attention to detail be carried out. Many other bleaching solutions have been tried without success. Peroxide of hydrogen has proved too slow a process in my hands, though its bleaching properties are held in high esteem by the gentler, and would-be fairer sex.

The solution I recommend owes its bleaching properties, I am told, to the presence of euchlorine, which is a simple mixture of chlorine and chloric-dioxide.

$$8KClO_3 + 24HCl = 8KCl + 9Cl_2 + 6ClO_2 + 12H_2O.$$

Bleached sections show, upon examination, a single layer of columnar, not flattened, cells, underlying the pars ciliaris retina, extending from the ora serrata to the bases of the

ciliary processes. The rounded, outer extremities of these cells, sometimes devoid of pigment, appear separated from the lamina vitrea by a small interval, presumably a lymph channel. Their inner ends can be distinguished from the cells of the pars ciliaris retina by a delicate, crenulated line. Between the individual cells slender, sustentacular fibres can be seen standing on the membrane of Bruch by their triangular bases. Around the ciliary ridges these cells are cubical, and contain comparatively little pigment, especially those at the summits of the ridges. This single row of columnar cells throughout the length of the pars non-plicata, I have seen only in the ciliary region of glaucomatous eyes, yet I consider it a normal condition.

In healthy eyes, those removed for injury or post-mortem, they are in some parts seen to be reduplicated by doubling outwards similar to the backward folding of the cells constituting the pars retinalis iridis.

This condition of the cells on the back of the iris appears dependent upon the movements of the latter; such an active membrane must have some arrangement of this kind. The more dilated the pupil the more corrugated would be its epithelium, and the converse.

Similarly, we must expect changes in the epithelium of the ciliary region resulting from the act of accommodation. In eyes in which I have found the pigmented epithelium thrown into folds, I have also found the lamina vitrea wrinkled, whereas in eyes in which only a single layer can be seen, this membrane of Bruch is found taut. The only inference that can be drawn from these facts, is that they depend upon the contraction of the ciliary muscle. The forward direction of the cells of the pars ciliaris retina, and their intimate relation to the fibres which form the suspensory ligament of the lens, will account for the absence of folding in them. It is probable that they elongate during accommodation. If, therefore, we find

the pigmented epithelium a corrugated membrane, it is due to the rigor mortis state of the ciliary muscle just as we often see similar post-mortem contractions in the unstriped muscle fibres of the intestinal tract.

Before I discuss the existence of true ciliary glands, allow me to make a few remarks on the apparent glands of this region.

How very difficult it is, as has already been pointed out, to make a complete antero-posterior section of a single ciliary ridge without passing through one or other of its side-walls, owing to their sinuous outline. Consequently, a closed space, or one opening by a narrow neck, may often be seen lined with both layers of epithelium, and may be mistaken for a gland. Again, the extensive folding between the processes may simulate a racemose gland, though here also both layers of epithelium are seen. Greef thought that Collins, in his communication, referred to these apparent glands, and, therefore, objected to the term "glands of the aqueous humour," preferring the expression, "secreting surface"; it is only an infolding of the surface epithelium and not a true glandular formation. I might, incidentally, mention that he is not alone in this wrong impression. A section through one of those cæcal diverticula of Brailey, would also be rather deceptive.

Another cause of error is the appearance of the pigmented columnar epithelium in the pars non-plicata, for each cell looks like a small, simple, tubular gland, owing to its outer extremity being rounded, sharply defined in many instances by the pigment, and forming with other cells a row of finger-like processes. The out-folding of the pigmented cells from the contraction of the ciliary muscle already described, is one that may also easily give rise to false deductions.

With so many fallacies and pit-falls, can we in safety acknowledge the existence of any true glands? Certainly we may. The tubular and pyriform glands described by Collins as

existing at the junction of the plicated with the non-plicated portions of the ciliary body, are true glandular structures, though not many in number or very large in size. They are cellular outgrowths from the pigmented epithelium, are flask-shaped or tubular, and some contain a lumen. Similar glands are seen between the ridges, and one or two in the pars non-plicata. It must be quite understood that the cells of the pars ciliaris retina do not participate in this outgrowth. Also, these glands in the natural state are deeply pigmented.

Beneath this melanotic epithelium the lamina vitrea is seen continuous with that of the choroid, and traceable as far as the ciliary ridges, where it becomes lost to view. Between this membrane and the ciliary muscle, a connective tissue framework accompanies the vessels, nerves, etc. The vascular arrangement may briefly be described thus: Around the circumference of the eye-ball, near the base of the iris, within the ciliary body, there is an arterial circle from which branches proceed to each of the ciliary ridges and break up into a free, inosculating plexus of fine capillaries on their walls. A venule forms at the summit of each fold and, passing backwards along its free border, carries the venous blood away to the choroid. There is no similar dense, capillary network in the pars non-plicata; in fact, this region is more poorly supplied with blood than in any other part of the tunica vasculosa of the eye-ball. I have come to these conclusions mainly from the examination of injected eyes of albino rabbits, and so cannot be dogmatic; but I feel convinced from the examination of a number of specimens, that this region is equally ill-favoured in the human eye. Nicati is of opinion that the vascular arrangement of the ciliary processes is similar to that of the choroid. To quote his words, "the ciliary epithelium rests on a layer of vessels—an expansion of the chorio-capillaris." Judging from analogy, it would not be right to accept this statement. It is not at all in accordance with what is seen on examining the injected tunica

vasculosa of an albino rabbit on the flat; the sudden cessation of the chorio-capillaris and venous system of the choroid at the ora serrata is a most striking feature. The blood circulation of the choroid is quite characteristic. Differing materially from that of the iris and ciliary body, and, stopping abruptly as it does at the ora serrata, must be for some special retinal purpose. This, however, Nicati does not allow, for he says that the chorio-capillaris lies between two impermeable membranes, the lamina vitrea internally and the intervascular membrane of Sattler (tapetum) externally, and any exudation from this capillary plexus must of necessity pass forward to the ciliary region, and there by a process of metabolism be converted into the aqueous humour. He boldly states that Sattler, with all other authors, is wrong in attributing to the chorio-capillaris the part of nutrition of the retina. This new origin of fluid to form the aqueous humour, seems too theoretical to gain universal acceptance. The fact that such a close meshwork of large capillaries exists over exactly the same area as the sensitive retina, and that it is especially fine at the back of the eye-ball near the yellow spot (Schafer[1]), it seems more feasible to accept Sattler's hypothesis. Extensive choroidal changes may occur without much alteration in the tension of the eye; if, however, cyclitis be present the tension is raised, and the subsequent atrophy of the ciliary body is attended with diminished tension. These points are certainly in favor of the fluid being derived from the blood-vessels of the ciliary processes, as suggested by Mackenzie some years ago, rather than from those of the choroid.

Why should the membrane of Bruch be considered an impermeable structure, for similar membranes, the capsule of the lens and the hyaloid membrane, allow fluid to permeate them?

Does the tapetum between the two vascular strata of the choroid in some mammal, or the analogous, iridescent epithelial

[1]Quain's Anatomy, Vol. III. Part III. 1894.

cells of Sattler as seen in fish, exist in man? No similar membrane can be seen with the microscope, and its presence seems more than doubtful.

What part the pyriform glands of Collins take in the secretion of the aqueous fluid, is a point worthy of great consideration, for this function has been attributed to them by their discoverer. He bases his arguments partly on histological, and partly on pathological grounds.

The existence of a few glands in the ciliary body appears to be the sole evidence that can carry any weight in favor of his theory. I emphasize "few," because I have never been able to find the numbers stated by that author, who says: "Their number in any one section it is impossible to count. The number in the whole circumference of the globe must be something enormous."

Now I find, with the exception of two or three glands in a single section, no other structure that can be considered glandular in the pars non-plicata, which, according to him, is a conglomeration of glands. I of course exclude the immediate vicinity of the ciliary processes.

He may mistake the folding of the epithelium, which I attribute to the contraction of the ciliary muscles, for glands.

The simple columnar cells, which resemble somewhat tubular glands while loaded with pigment granules, cannot possibly give rise to error when bleached.

Two fundamental facts justify the exclusion of these cellular plaits from the type of ordinary glandular formations: First of all, the absence of any corresponding depression in the basement membrane, which, here, is the lamina vitrea; secondly, the occasional absence of any such folding or reduplication of these cells. Besides, a gland must surely be composed of more than four cells, as seen on section, which these so-called glands never do. Would we not expect a free network of capillaries

beneath these apparent glands if they were real? but we have no proof that such exists.

There is not sufficient time to allow me to enter fully into the pathological evidence which is brought forward. I must, however, deny that ridiculous theory of glandular hyperplasia in cyclitic membranes, enuciated by Alt and accepted by Collins.

Can it be possible for a gland from the stimulus of inflammation, to sprout out into a mass of inflammatory material and form a more highly organized structure than the original? Is it not contrary to all pathological dicta? Where can we find a parallel phenomenon? It is truly absurd, and cannot be contemplated as based on sound, scientific principles. Some of these exuberant glands are, in reality, due to folding of the epithelium, both pigmented and non-pigmented, from the cicatricial contraction of the inflammatory material in the process of organization. Others, and the variety to which Alt seems to refer, are those peculiar, branching and anastomosing cellular rods, or columns, seen in so-called cyclitic membranes; some cells containing pigment, others not. Such are more sensibly explained by assuming them to be newly-formed, lymph channels congested with leucocytes, performing a useful work in clearing away the melanotic debris of the disintegrated pigment cells from the site of the recent inflammatory battle. In other words, a process of phagocytosis, or undertakers burying their dead.

Do the ciliary processes, or the ciliary glands, secrete the aqueous fluid? One ciliary process must be capable of secreting many times the quantity of several glands, if size is to be taken into consideration. It is exceptional to find more than half a dozen glands in any one section.

Let it be assumed for a moment that they do constitute the intraocular secretory apparatus. Then, what use are the ciliary ridges? Their existence must not be wholly disregarded; in

fact, a more important function should be assigned to them in proportion to their magnificence. But we cannot do so. Placing aside the question of size, it is possible to bring forward a more substantial argument against this glandular theory. Why did not Nicati recognize them? He makes no mention of them in his paper. The reason is obvious. He made his experiments and examinations chiefly on the eyes of albino rabbits; in them no such glands can be found, yet they are not devoid of an aqueous humour. What then secretes it? Presumably, the ciliary processes which do exist.

Just a few words about the experiments of Greef. He tapped the anterior chamber of a number of rabbits, and examined them at different periods, afterwards, and found the cells lining the processes, epecially those on their anterior aspect, distended with coagulated fluid similar to the coagulable fluid which takes the place of the aqueous humour in the anterior chamber shortly after that cavity has been evacuated.

I have had the opportunity of examining the ciliary region of eyes which have been excised within a short time after perforating injuries. The cells of the ciliary ridges show a similar condition to that described by Greef. The epithelium appears to be distorted with vacuoles which, no doubt, are aqueous droplets. They, however, have not taken, even faintly, any stain, so cannot be considered coagulated fibrin. This condition is, no doubt, an exaggerated form of the normal secretory process.

Seventy-four years ago, [1]John Vetch wrote as follows: "In assigning to the ciliary processes the office of a gland, I was not aware that Dr. Young had already supposed that this might be their use, if the production of the aqueous humour required such an explanation. As the secretion of the fluid, after closure of the pupil, proves that it takes place behind the uvea, this destination of the ciliary processes is rendered very probable."

[1]A practical treatise on the Diseases of the Eye, by John Vetch, London, 1820.

There has arisen since that time little substantial evidence to overthrow Dr. Young's theory, and I sincerely believe that it is through the mediation of the ciliary processes, alone, that the aqueous humour is formed.

Having, in the main, refuted the hypothesis expounded by Collins, I should not feel I had done so without prejudice if I could not attribute some function to the ciliary glands which have been robbed of their colours. I have found them to exist in greater numbers, and of large size in dark-brown eyes; in blue eyes they are present, but much less prominent, and in the eyes of albino rabbits, I have already said, they are not to be found. It seems to strike me forcibly that they control the amount of pigment in the eye and regenerate it, as nature demands.

"CATARACT EXTRACTION AN OFFICE OPERATION." SOMETIMES. (AND MOST OF THE TIME.)

By Dr. J. W. C. LOVE, of the City of Mexico,

Formerly Oculist and Aurist to the Civil and Military Hospitals in cities of Morelea, and Hopil Loreta, at Vera Cruz, Mexico.

The most of my cataract operations have been office operations, since 1882 (twelve years and a half), and now I will describe my procedure, as I think it necessary to justify myself.

With pupil thoroughly dilated with atropine, and thoroughly anesthetized with cocaine, I place the patient in a straight back chair, facing a good light. With speculum in, I make the incision shown in Figs. 1. and 2, and finish as shown in Figs. 5 and 6. All figures or cuts represent right eye, and operator standing behind, using right hand, and as I am ambidexter, of course, apply same principles operating on the left eye.

Fig. 1 shows the narrow knife passed through cornea to pupilary edge of the iris; then I raise the handle up and out from the face (Fig. 2) and dip into edge of lens capsule, and gently pass it through the anterior lens capsule, and then out and over edge of iris, as seen in cuts Nos. 3 and 4. Then lowering knife handle, finish section in cornea as well as lens capsule, at angle shown in Figs. 5 and 6.

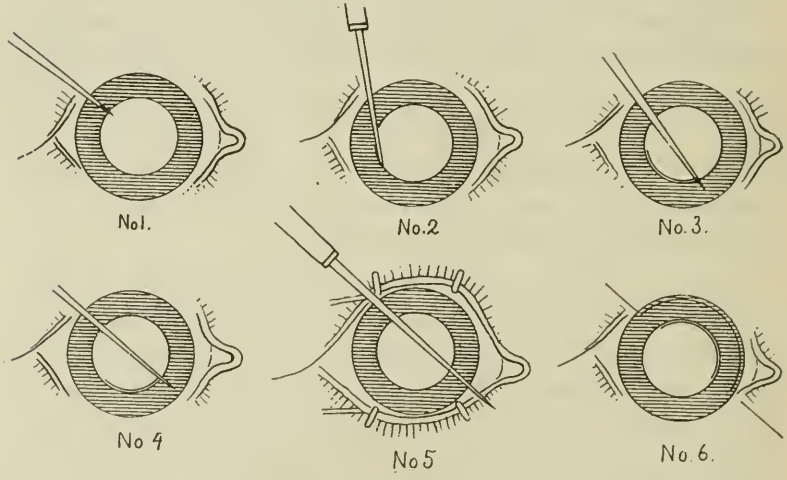
Figs. 2, 3 and 4 show heel of knife stationary, and the point passing gently curving and cutting through capsule, with point out over the edge of iris.

Fig. 3 shows point advancing until it is in position to cut out through cornea, as shown in Fig. 4, and then finished as shown in Fig. 6.

The lens I take out in usual way, never performing iridectomy, unless there are iritic adhesions. The four-fifths or more of the front side of lens capsule cut away, falls down, and in time

becomes absorbed or is completely out of the way of the pupil and vision, when iris regains its natural position. Getting the cataract out is perhaps the most particular part of the operation, and especially to take out all lamina or lenticular matter; and the following is my method:

I take a rubber lens scoop, curved on the back about half an inch up towards the handle, with the hollow of the scoop on inside of the curve; and with back press gently on cornea until lens makes an edge "presentation" and comes out, and then lift it away with the hollow side. If any lens



matter is left, I take it out with a fenstrated scoop, and if any particles fall back that I can't get out in this way, I use a very small pipette (medicine droper) curved, with very smooth point, and inject a *sterilized saturated* solution of Boracic acid in, *gently* wash them into view, and suck them out with same. I *never hurry* in the operation. When ready to cover or dress the eye, I contract pupil with a weak solution of eserine (it contracts pupil so that prolaps of iris is *always* avoided with me), and allow its effects to wear gradually away. Then on about third or fourth day, I dilate pupil with atropine to draw iris away

from any capsule substance. It is *rarely* necessary, after this operation, to have to do a secondary or needle operation.

"In so precious a thing as vision," as Dr. Lucian Howe puts it, in your September Record, I do not think "fads," should have any part; and will now describe my process of bandaging the eye, and the liberty I give my patients! I cannot believe it is safe to let them go "one eye on it" (one eye open), although I give my patients generally more liberty than anybody except Dr. Julian Chisolm, of Baltimore, whom I have seen operate most beautifully, pinching the anterior capsule off, in the prettiest manner I have seen or read of. The principal part being finished, I think bandaging the eye a most important part. After cleansing the eye with anticeptic solution (Hydrargiri Bichoridi usual strength), I then put a piece of muslin wet with the same, over both eyes, then small pieces of cotton-wool, according to depression of eyes and projection of nose, frontal and cheek bones, and then a bandage made thus: —I make a bandage long enough to go around the head three times, about two inches wide, the inner third of black calico, to dispense with dark or shaded room, the other two thirds of nuns veiling or light flannel, a little elastic, if in cold weather, or bobinet if summer. The black third I put next to the eye (over the cotton), straight across and around; the next two thirds I wind around the head twice, letting the first round cover one ear (nearly) low down on one side and high up on the other, and the second round the reverse to this, thus making the two white flolds of the bandage form a long X horrizontally over the eyes and nose. Then I put a piece of elastic cloth, nuns veiling or bobinet, about as wide as a hand, across the top of the head, and pin the four corners, thus forming a close fitting cap. I spread the bands apart behind, and put in a stay-pin to keep in place. I do not put this band on the eyes tight, but by means of the cloth over the head, pinned at its

four corners, at about equal distances around the head, it protects eyes better than any other way, I think.

Now the curtain rises on the new act (to some). I send them home or to hotel from my office, with some one leading them carefully.

Thus the "Cataract Extraction an Office Operation," has been practiced with slight modifications by me for twelve years and more.

When I came to Mexico first, twelve years ago, there were few Oculists, and I was, and am now, often called to adjoining cities and towns, where there are none; and as there were a great many cataracts, especially among the poor, knowing it was the first, and probably the last opportunity many of these poor would have to receive sight, I operated and treated them as above described. I want to say that I make exceptions occasionally—in cases of feeble old people, when the patient is very nervous, afraid or unruly. And I want to say further, that I do not treat these people slightly. Fashions, or what one Oculist does, may not be the best for another; and no one ought to venture, except those with experience, and then always remember "so precious a thing as vision is at stake."

Excuse me if I seem presumptuous when I say, in substantiation of my preferred operation and treatment, that I am not a "beginner," having operated upon eight cataracts in one day, and forty-seven in one month.

When the eye is closed, the angle of incision or section in cornea is thoroughly covered with the natural cover of the eye, and the curvature of the cornea is preserved, and lips of section are kept in exact place by the curvature of the tarsal cartilage; and thus I have less astigmatism, and when present, less in degree than most Oculists. I heard in person, one of the writers in the Record, who likes, and often performs, cataract operations, and lets his patients "go one eye on it" after the

operations, complain of his great per cent. of astigmatism, after the operation. To me the cause seemed clear. I want to call attention to one important fact. Though a patient should imprudently take bandage off, and open the eye; or as Lucien Howe, M.D., in late number of the RECORD, says: "just to fix the bandage a little," they are not liable to do harm, or, not as liable as in the horizontal section. Where the horizontal section has been made, any elevation of the lid can, and some times does, cause wrinkling of the flap, breaking or partially interrupting the healing, and sometimes causing prolaps of iris. After the oblique section, the edge of the lid moves over the part almost at right angles to the section, and finds no edge or unevenness to catch to, to cause breaking up of adhesions or wrinkling of the flap. But bear in mind, the kind of bandage I use is not easily removed, and its fixture and appearance to the home, or other folks inclined "just to fix the band a little," will give the impression that it is fixed to stay, or to be removed only by the Doctor, and they will not be apt to meddle with it.

It is not often necessary to put more than one instrument in the eye (the cataract knife) especially if it is a senile (or ripe) one.

SCIENTIFIC PROCEEDINGS OF THE SECTION OF OPHTHALMOLOGY, A.M. ASSOCIATION, SAN FRANCISCO MEETING, 1894.*

ABSTRACTS OF PAPERS; DISCUSSIONS IN FULL; REPORTED BY G. H. PRICE, M.D.

[*Continued from page 199, current volume.*]

RETINOSCOPY AS A CRUCIAL TEST IN MEASURING ERRORS OF REFRACTION.

By B. ALEX. RANDALL, A.M., M.D., Philadelphia, Pa

After stating in his paper that he used the plain mirror in his retinoscopic work, he then defined the limits within which it had the greatest practical value. The following two paragraphs we quote from his paper:

"In my routine examination of a patient, retinoscopy follows the subjective tests, for far and near, of the vision and muscle balance. In the moderately darkened room, with an argand burner shaded by a metal chimney presenting an opening an inch and a half in diameter, the light is thrown by a plane mirror from a distance of three or more meters, while the patient gazes at a dark surface behind me. Under these circumstances a medium pupil (four to six millimeters) is almost invariably obtained, in which there is rarely any difficulty in seeing and determining the motions of the shadows. M. and H. are thus readily differentiated, with the presence and axis of any astigmatism, and a rough quantitative estimate can be made from the distinctness and rapidity of movement of the shadow. Mixed astigmatism, usually the most difficult error to uncover, is

*This report is taken largely from *The Journal.*

by this method the easiest and most unmistakable. With very narrow pupils, hazy media or unsteady patients, some little difficulty may compel a closer approach; but if the examiner's head be a little lower than the patients, this can generally be avoided and the test satisfactorily made at even greater distances. As the pupil is not strongly illuminated, the mirror must have an ample sight-hole, free from annoying reflections.

"For this, some painstaking in the illumination may be required in order to give readily the desired accuracy. Theoretically, we should obtain the emmetropic appearance at a distance of four meters only when M=.25 is present. Practically this rarely holds good, probably because the pupil is wide enough in most cases, even when no mydriatic has been employed, to give a peripheral refraction higher by thus much than that at the pupillary center, and also because we usually compare results with the subjective correction for six meters, which is slightly over-correcting for distance. I look therefore, for a central, comma-like shadow, rotating or varying with the shifting light, yet moving neither against nor with the motion of the light upon the face. If this be obtained with+.75, for instance, it will be found that +.90, the old 42, gives a distinct myopic shadow moving in the pupil against the mirror, while +.65 is as clearly an under-correction and gives a shadow moving with the mirror. So it is fair to claim that errors as small as 0.1 in any or all meridians can be recognized by this method under favorable conditions. Indeed, I have seen cases where 1 D. was as clearly too strong as .90 was too weak; and one could assert that there was not so much as .05 D. of astigmatism. This minuteness of measurement is as true of one meridian as of all; and the axis of any uncorrected astigmatism can generally be determined within 5 degrees with the same ease and accuracy. It seems just to say, then, that this is the most minutely exact measure at our command."

Following Dr. Randall, a paper on

THE STATUS OF SKIASCOPY.

By Dr. H. V. WURDEMANN, A.M., M.D., Milwaukee, Wis.,

was read. The Doctor claimed that, of all the methods of testing for errors of refraction, skiascopy occupied the first place. We quote the following paragraphs from his paper:

"From the perusal of recent English and German literature, it would seem that even though the shadow test has been in common use in this country and France for a decade, the profession on the other side has not taken up the method to the extent that we have. In this country, I think that it may be said all the progressive workers use it and do not consider the refracting of any case complete unless skiascopy has been called into service. Within a couple of years the acquisition of this method will be deemed as necessary by the foremost specialists of all the world as it is here. Six years ago I was astonished by the ignorance of the German oculists regarding skiascopy, as it was then, as now, foremost in my mind as being the principal objective method of measuring the eye. It is different now, and I understand that the shadow test is being generally taken up even in the clinics of Germany. Personally, I voice the opinion of its value given by your committee, and use it a number of times a day in my private practice.

"In what cases should the shadow test be used? I should say in *all* patients for whom lenses are to be prescribed. I hold it to be an absolute necessity for the scientific refractionist to know in every instance the total refraction of the eye before putting on lenses. I would no more think of fitting spectacles to a presbyopic patient, by trying lenses at the near point alone without knowing his refraction, than I would of prescribing glasses in ametropia by one or more of the objective tests without recourse to the subjective method for confirmation.

"The few instances in which skiascopy is unreliable or inap-

plicable are limited to those cases in which a definite visual zone cannot be isolated. These eyes are the exception and yet where carefully used, in irregular corneal curvature, in conical cornea, in partial lenticular or corneal opacities and in very high grades of astigmatism, skiascopy is of value as a confirmatory test. 'In nearly all eyes the result of careful skiascopic examination will give the best obtainable vision.'

"For skiascopic results to agree with those elicited by prolonged subjective tests, the examination should be conducted in the visual line, the refraction of the macula only being measured. Skiascopy measures the static refraction of the eye when applied in the visual zone, exactly as it is at the time of the examination, whether modified by the accommodation or not. Thus, in young persons, as a rule, a mydriatic should be used. Skiascopy is of value in the proving of measurements found by the other methods, just like the proving of an example in vulgar fractions, by again applying the shadow test with the combination of lenses decided upon from the trial case. It may thus be used as a check upon the other methods and upon itself."

In closing his paper the Doctor made reference to the skiascope invented by himself, and demonstrated its utility.

The title of the next paper was

THE VISUAL ZONE OF THE DIOPTRIC MEDIA AND ITS STUDY BY SKIASCOPY.

By EDWARD JACKSON, A.M., M.D., Philadelphia, Pa.

We quote the following paragraphs from his paper:

"The practical importance of the study of the visual zone, and the relation of its refraction to that of other parts of the dilated pupil, is very great, and the facts revealed by such a study will powerfully influence the prescription of correcting lenses. How great this importance is, and how much the practice of lens prescribing is likely to be influenced will only be appre-

ciated by one who has made at least a few careful studies of this kind. Leaving it aside for some future communication, I wish simply to speak now of essential methods of such study.

"The area of the pupil that may be regarded as having the same refraction depends on the exactness or minuteness of discrimination that it is desired to make. If one says roughly a certain eye is hyperopic or a certain eye is highly myopic, this may apply to the whole dilated pupil. If he says the eye has so many dioptres of hyperopia or so many dioptres of myopia, and is willing to ignore variations of less than 1 D., what he says may apply to the whole of the pupillary area as it exists under conditions of ordinary illumination. But, if he desires to be more discriminating, to recognize the smallest differences of refraction that can be recognized by skiascopy, he will find, in almost all cases, that such a pupillary area presents marked variations. And in a certain number of cases of eyes that would not ordinarily be regarded as exhibiting conical cornea, or marked irregular astigmatism, the area of the pupil even contracted by strong light and for near work, will show variations in refraction of the greater part of a whole dioptre.

"From these facts, it follows that the accurate application of skiascopy, the careful study of the visual zone and the relation of its refraction to that of the surrounding proportions of the pupil is a study of the *movements of light and shadow in small areas* of the pupil, not across the pupil as a whole.

"To study a small object one must bring it near the eyes; hence such studies of the pupil must be made within a comparatively short distance. Distinct movements in an area three millimeters in diameter cannot be conveniently studied beyond the distance of a meter and can be most readily studied at a considerably shorter distance. There are not a few eyes capable of visual acuteness quite up to the normal standard, in which the light that goes to form definite images enters through a smaller area of the pupil than this, and the true refraction of

the eye, therefore, can be studied only at a shorter distance, a distance of one-half or even one-third of a meter.

"Again, to have a distinct area of light in such a limited space, it becomes highly important that the source of light be small. Such a study cannot be made unless the ordinary lamp flame be covered with an opaque chimney with a small aperture. For my own work, with the plane mirror, I have for the last year found an aperture giving a circle of light five millimeters in diameter most satisfactory. With such a source of light, properly placed close to the mirror, and the sight-hole of the mirror not more than two and a half or three millimeters in diameter, the study of the movement of light and shadow is as easily made at one-half meter, revealing the differences of refraction in different parts of the pupil, as is the rough study at one meter distance or more, by the methods ordinarily described which have so frequently misled the surgeon."

DISCUSSIONS.

At the close of Dr. Jackson's paper the three kindred papers were discussed together. The discussions we give in full:

DR. F. B. EATON: In the employment of the several objective tests under discussion without mydriatic, I prefer to begin with skiascopy, since a certain amount of accommodative adjustment brought about by the subjective tests and by the fixation by the patient of the tube of the ophthalmometer.

I employ the ophthalmoscopic mirror with Dr. Wurdemann's skiascope and without a mydriatic. If the pupil is small, the patient must fix some object considerably to the right or left that a reflex may be secured; hence the importance of a very dark room and covered flame. I occupy a middle ground between the extreme views of those who eulogize, and those who take as partial only, the finding of Javal's ophthalmometer. I believe that many of the discrepancies between its findings and those under mydriasis result from imperfect illumination, and because the patient's head is not level. This latter defect can

be remedied best by the American model, which provides a horizontal slit through which the observer can insure the levelness of the patient's brows with the top edge of the slit. Again, the patient must fix a point on the anterior lens of the instrument, which is in its axis, since then the observer measures the cornea at that portion cut by the visual line.

Dr. S. B. Davis, Stockton, Cal: In my use of the ophthalmometer, I have found that I had better results in most cases by placing the axis at 90 degrees, if the instrument showed it to be near 90.

Dr. Simonton, San Jose: While the objective methods mentioned are theoretically within certain limits correct, yet it is a fact in practice that many mistakes are made by those relying almost wholly on these methods. Many of these cases require refitting, and ordinarily by use of the old trial case. In-as-much as the various objective methods afford latitude for the exercise of much judgment, it is evident that all ophthalmologists are not equally endowed with skill and acuteness of judgment and this, accompanied by usual haste, results in many mistakes.

Dr. R. D. Gibson, Youngstown, Ohio: The results are not always the same in the same eye. A case showing plus .50 D. astigmatism may, after some months of over-taxation in school, return with conjunctivitis and an increase of the corneal astigmatism. This increase I have seen reach as high as 4 D. and after relieving the conjunctivitis and blepharospasm, the high degree of astigmatism vanishes and the results of the first examination found to be correct. Again, I have found astigmatism for which I have prescribed correcting lenses with great comfort and satisfaction, and after some months or a year, find no astigmatism, and only a spherical correction necessary.

Dr. Fred Baker, San Diego, Cal: To repeat much that has been said, I wish to plead for greater charity in our treatment of our brother practitioners, when we are called upon to over-

haul their work. I have grown more charitable since finding that as large a share of my own cases show slight changes in a second test made after six months or a year, as of those which come to me from other practitioners, though I know that the first test of my own patients were carefully made. Every one must find that certain patients show these changes after a time, which must be due to the relief of strain given to the ciliary muscles by the wearing of the first pair of glasses.

As to objective tests, while it must be true that all of us in the rush of business neglect to give sufficient time to all patients, still it must be admitted that the oculist who combines most methods of testing will do more accurate work than he who uses only one. I have yet to meet personally any ophthalmologist who has given any of the objective methods a full and careful trial, who has not admitted the practical value of the method.

Dr. W. E. Briggs, Sacramento, Cal: I agree with Dr. Riley in the statement that the trial test case will be our last and most important means of ordering glasses; but the more the profession uses the objective methods, the more general they will become. I believe, in time, all ophthalmologists will use both skiascopy and Javal's ophthalmometer. These methods of testing are not always necessary, but cases are common in which we cannot readily obtain a result without some objective test. As for myself, I would feel quite at a loss to make my examinations without the ophthalmometer and skiascopy.

Dr. Albert R. Baker, Cleveland, Ohio: I am pleased with the report of the committee in that they have given great prominence to skiascopy as an objective test for the determination of ametropia. In 1881-'82, I spent considerable time in the Royal London Ophthalmic Hospital (Moorfields), about the time skiascopy was first introduced in that institution. Probably one-half of the patients applying for relief could not read and we found this objective test of great value in these cases.

Upon returning to this country in 1883, I published several papers on this subject, but as they received but little consideration I became somewhat discouraged in attempting to secure a hearing. I was greatly pleased when Dr. Jackson took up the subject and succeeded in placing this objective test where it belongs as one of the most valuable. No doubt the present excellent report will serve to make the use of skiascopy more general.

Dr. L. R. Ryan, Galesburg, Ill.: The use of the old trial lens should not be discarded. The thoughtful physician and progressive man will use all the methods at his command. It is in the majority of cases a matter of personal preference. The man who studies skiascopy carefully and uses it extensively can do better work than one who devotes little time to it. The same might be said of the other methods of refraction. When we consider that the majority of cases of refraction are simple; that is, the axes of astigmatism are regular either at 180 degrees or 90 degrees, and that the patients are people of average intelligence and perception, by the use of the trial case we can fit glasses more rapidly and accurately. We have all the benefit of the patient's intelligence as to clearness of outline and ease of glasses. By a careful study of subjective symptoms one can tell almost by intuition what the refractive error is, and in the vast majority of cases can make a rapid fit. In suitable cases, however, skiascopy and all the other methods are excellent, and should be used, but the old trial frame still has a place and an important one.

Dr. H. V. Wurdemann, Milwaukee: Our branch of therapeutics is the most scientific of all the branches of medicine, and this refractive work is, or should be, the most accurate, as it is based upon physical and mathematical data. The physician who bases his glass fitting upon such ground necessarily does better work than those who depend entirely upon unskilled testimony of the patient. In the opening sentence of the report of your

Committee, the fact was stated that the objective tests were a necessary preliminary to the subjective method, and the latter is not to be done away with, but is the court of last resort. We may even go further with the subjective method, and say that the final judgment of the patient himself after he has had our prescription filled, is a highly important point. Thus we cannot do without the trial case. The advantages of the objective methods have been set forth in your committee's report and I need not repeat them here.

One of the most important offices of the objective method is the clearing up of doubtful points excited by the subjective examination. Thus at the trial case a patient may accept equally well one or any of the following combinations:

Plus .50 sph. ⊂ plus 1.25 cyl.

Plus .75 sph. ⊂ plus 1.00 cyl.

Plus 1.00 sph. ⊂ .75 cyl.

Without repeated sittings, by the trial set alone it would be difficult for the operator to decide which would most nearly neutralize the patient's error of refraction. Skiascopy settles the question immediately to the skilled operator. There has been some question raised as to the fixation of the patient's eyes. The examination should be conducted as nearly as possible in the visual line of the patient. Fixing the aperture of the mirror would do this, yet reflection of the same on the anterior surface of the cornea would confuse the operator, and when used without a mydriatic, the bright light falling most directly upon the fovea would tend to produce iritic and ciliary contraction. For general purposes it is better to have the patient look over the operator's shoulder at some distant object. I accomplish this purpose by having curtains at the door of my dark room, through which, when slightly opened the patient looks at a wall four meters distance. This method secures relaxation of the subject's accommodation.

In regard to the order in which the tests should be taken up,

I consider it advisable to go from the coarser to the finer tests, as the former give valuable hints as to the finer measurements to be expected from the latter; taking up the examination in the following order:

Visual acuteness and astigmatic lines, keratometry, direct ophthalmoscopy, skiascopy, the subjective methods and then skiascopy again for corroboration of the whole test. Naturally, as in any other skilled work, much depends upon the observer, but even to those who, either by reasons of physical disability or mental hebetude, have not been able to acquire sufficient skill for the finest work, these objective tests prove of material benefit.

Dr. Lewis H. Taylor, Wilkes-Barre, Pa.: In regard to the use of the ophthalmometer, I can say that I have recently come to regard it as a most useful aid to the rapid determination of astigmatism. A few years ago in a discussion of this subject we heard the ophthalmometer referred to as an instrument that might be regarded as an interesting toy, and which would soon be relegated to the lumber room as something too useless to occupy the busy oculist's valuable time. Now, Mr. Chairman, I do not so regard it, but I find it daily of more and more value. We mistake greatly if we regard it as an accurate determiner, if I may use the word, of the exact refraction; it is only an aid, but a most valuable aid. I formerly regarded the subjective test with atropia, as the only method worthy of serious consideration, but now I am free to say that I use atropia far less than formerly. Ophthalmometry will not entirely take the place of subjective tests, nor will any other method, but I have frequently had patients return who had been wearing glasses with reasonable comfort for whom I could get a much more satisfactory correction than by the old method. The difficulty too often lies in the fact that some one devotes exclusive attention to one method, and claims too much for it; another tries it superficially and does not get such good results; he denounces

the method and discards it. Both are in error; the middle ground here as elsewhere is the safest.

Dr. A. J. Erwin, Mansfield, Ohio: I do not believe it important what instrument, or which of the methods in use, be employed in working refraction, so that we are only expert in our method, and expertnesss is necessary in any method in order to have good work. In regard to the varying of the degree of astigmatism, in any case it is doubtless due to the power of the accommodation at the time of the examination.

Dr. Jackson, in closing the discussion, said: Dr. Randall, in using skiascopy to obtain his extremely accurate results at greater distance than I have advised, works with the undilated pupil, thus excluding the extra-visual zone. Yet even then he did not find it necessary to make the theoretical allowance for distance. Now it is a fact that in the majority of cases the refraction of the extra-visual zone shows higher hyperopia or lower myopia than the visual zone, so that for this extra-visual zone such an allowance is out of place. Evidently under these conditions, the results have been appreciably affected by the refraction of this extra-visual zone.

In regard to the illumination of the ophthalmometer: it should be remembered that we are studying the reflection from a transparent membrane, and that its distinctness will depend both on the illumination of the reflected object, and on the absence of light transmitted from behind the cornea, and for this reason it is almost as important to keep the eye shaded as to illuminate the disc and mires.

In reply to the questions as to the percentage of cases in which the ophthalmometer can be relied on for the prescription of lenses, I have recently, in 500 eyes, found that the corneal, corresponded exactly with the total, astigmatism in only 6 per cent. In about 20 per cent. the amount was almost exactly the same; in 60 per cent. the difference was not over 0.5 D.; in

52 per cent. the direction of the meridian corresponded exactly, and in 80 per cent. within fifteen degrees.

As to variations in astigmatism from time to time, it must be recognized that these do occur. They might be expected in progressive myopia, but they occur with the hyperopic astigmatism as well. My attention was first called to this in my own case where the astigmatism has increased from 0.05 D. to 1.75 D. Some years ago I published seventeen cases that I had been able to follow from year to year, with determination under mydriatics.

[*This report will be continued in the next issue.—Ed.*]

SCIENTIFIC PROCEEDINGS OF THE SECTION OF OPHTHALMOLOGY OF THE BRITISH MEDICAL ASSOCIATION, BRISTOL MEETING, 1894.

[*Report continued from page 177, current volume.*]

DIAGNOSIS OF THE PARALYSES OF OCULAR MUSCLES.

By Dr. E. LANDOLT, of Paris.

He thought that the diagnosis could be made with comparative ease and gave his method of examination: First he used the red glass and thus determined whether the diplopia was homonomous or heteronomous. Second, by moving a light, held a short distance in front of the patient, he determines in which direction the diplopia exists, and thus he can readily tell which muscle is at fault. In the third place he stated that the position in which a patient held his head frequently indicated the muscle that was at fault.

THE MAINTENANCE OF EQUAL ROTATION OF THE EYES AFTER OPERATIONS ON THE OCULAR MUSCLES.

By GEORGE T. STEVENS, M. D., New York.

In this paper Dr. Stevens presented his views as to the proper method of operating on the eye muscles, always dividing the operative effect between the corresponding muscles of the two eyes. He does this whether the indications are for tenotomies or advancements. He taught that a complete tenotomy should never be done in operating for any form of heterophoria; and said that such a procedure was rarely admisable even in cases of strabismus.

The next paper being on a kindred subject was read, its title being

EXPERIMENTAL INVESTIGATIONS OF EYE MOVEMENTS.

By Dr. J. S. R. RUSSELL, M.B.

The first series of experiments set forth were made by irritating the cortex; the second series were made on the cerebellum.

DISCUSSION

Of the papers read by Drs. Landolt and Stevens and Mr. Russell then followed.

Mr. Snell showed some photographs of patients who had one form or another of ocular paralysis, the position of the head indicating with a high degree of accuracy the affected muscle. He resorted to various tests, however, in arriving at his conclusions.

Dr. Savage (on Dr. Landolt's paper) said, that in teaching his classes during the last three years, he had resorted to a very simple procedure in determining which one of the recti muscles has paralysis. Holding a lighted candle in the median plane it may be seen as one. Moving it to the patient's right, diplopia results. The affected muscle is on the same side of the eye to which it belongs as is the double candle; therefore it must be the internal rectus of the left or the external rectus of the right eye, and the affected eye sees the candle that is farthest removed. Exclusion of one eye or the other quickly settles the question.

Suppose that it is one of the vertical muscles that is affected: we begin the test in the horizontal plane. If moving the candle up develops diplopia it is a superior rectus that is diseased, and it belongs to the eye that sees the higher candle.

Dr. Savage (on Dr. Stevens' paper): As to the etiology of internal squint, I will say that I believe that Donders was not wholly wrong when he taught that hypermetropia was the cause. While I do not believe that hypermetropia alone can

cause squint, I do believe that it is one of the causative factors in most cases. The impulse that must come to the ciliary muscles of hypermetropes is always attended by a corresponding impulse to the internal recti, developing what I termed a pseudo-esophoria, in a paper published more than a year ago. The chief factor in the production of internal squint is intrinsic esophoria. Neither one of these conditions existing alone might be able to develop a squint, but the one grafted on to the other may do so; hence an indication for treatment.

Another factor that may cause squint in esophoric cases is hyperphoria, as clearly set forth by Dr. Stevens in his paper.

While other factors might be mentioned, I think I am right when I say that the three great factors entering into the causation of internal squint are, pseudo-esophoria (caused by hypermetropia), intrinsic esophoria and hyperphoria, and that the greatest of the three is intrinsic esophoria.

In the treatment of internal squint the first thing to do always is the correction of the hypermetropia which takes away the pseudo-esophoric factor. If the patient is young, this alone will often result in a correction of the squint, although the intrinsic esophoria has not been altered. Likewise an operation for the cure of the hyperphoric condition will relieve the squint though the intrinsic esophoria remains.

In older cases I know the interni must be operated on, and in some cases the externi as well. I fully agree with Dr. Stevens that the operative effect should be divided between the corresponding muscles.

Mr. Wray presented a diagram of his own device by means of which he could easily detect whether the superior rectus or superior oblique is paralysed, likewise the inferior rectus or inferior oblique.

Mr. Doyne said that heterophoria greatly complicated any study of paralyses of the ocular muscles.

Dr. J. L. Thompson said that the patient, to prevent diplopia, instinctively closes the affected eye. This thus readily determined, the finding of the affected muscles was easy in most cases. In cases of internal squint he advocated the early correction of the hypermetropia, stating that, in his practice, many squinting eyes had thereby been straightened. He had put spectacles on squinting children at as early an age as three or four years. These cases taken early can be cured, while good results rarely ever follow in old and bad cases whatever may be done for them.

Mr. Tasswill said that in operating for squint he rarely ever got binocular single vision, and asked for the experience of others on this point.

Mr. C. H. Walker said it was easy to make a diagnosis of paralysis of the ocular muscles, and referred to, but did not describe, his method of procedure.

Dr. Risley said that anisometropia is so commonly found in cases of squint, he thought it must act as a causative agent. Whatever else is done for the cure of squint the focal error should be corrected.

Mr. Cross said that sometimes, in ocular paralysis, the patient selects the faulty eye for progression. In discussing Dr. Stevens' paper, he said that while some of us think that Dr. Stevens goes too far in his study of heterophoria, yet we all know that he has done great and lasting good. He (Cross) has never operated on a case of heterophoria. He believes that anisometropia bears a causative relationship to squint. The squinting muscle is too short, and the opposing muscle is too long, hence both of these muscles should be operated on (the one tenotomized and the other advanced) before going to the other eye.

Dr. Landolt said that strabismus is always a binocular trouble and therefore both eyes should be operated on so as to equalize the tension of the corresponding muscles. He opposed tenotomies and favored advancements.

Dr. Stevens commended Dr. Landolt's paper on the determination of paralysis of the ocular muscles, and then gave his own rule for distinguishing paralysis of the obliques from the same affection of the superior and inferior recti [see page 31, No. 1 vol. iv. of this journal]. He commended Mr. Russell's experimental investigations of the action of the ocular muscles. He astonished the greater part of his auditors by stating that he had never seen a single case of internal squint cured by the correction of hypermetropia. He did not believe that hypermetropia bears any causative relation to squint. As to the correction of squint, he stated that it was a most difficult procedure: that the operation should be done step by step, and that the aim should always be the production of binocular single vision. The cases of antipathy to binocular single vision always demand operation on the superior rectus of one eye, and sometimes the inferior rectus of the other eye. Often, in operating for squint, as the tension of one set of muscles is relieved another set takes on tension.

The next paper on the programme was

ON THE GOOD EFFECTS OF DRESSING ONE EYE ONLY AFTER CATARACT EXTRACTION.

By J. J. Chisolm, M.D.

This paper has already been published in full in No. 3, Vol. iv. of this journal, page 108.

The next paper was on

OBSERVATIONS ON SOME PHASES OF OPACITY AND LUXATION OF THE CRYSTALLINE LENS.

By J. L. Thompson, M.D.

In this paper he gave the history of some very interesting cases. First, he referred to cases of segmental lenticular opacities which occur downward and inward. When he first observed these cases he always told his patients that they

had beginning cataract, and shortly they would be blind, and would have to be operated upon. Experience taught him that, in the majority of these cases, these opacities did not extend to pupillary space but remained local, not interfering with vision. Finding such a case, he has now long been accustomed to make drawings in his case-book, so that, year after year, he might make comparisons. This period of careful study of these opacities has now covered twenty-two years, and has convinced him that such patients are not likely to become blind by involvement of the whole lens. The youngest person thus observed was forty, the oldest eighty-seven years. Usually it was in both eyes. These opacities, once in fifty, occur down and out. When they occur in upper part of lens the prognosis is not so favorable. He presented no theory as to the cause of these local, stationary opacities.

He also described a circular peripheral opacity of lens, which he said is not always senile, as he had once seen it at the age of twenty-three, and often as early as thirty-five years. This, too, is not likely to involve the center of lens. He then reported some interesting cases of ectopia lentis, congenital and acquired.

The next paper presented discussed

THE DIAGNOSIS OF THE CONTAGIOUS FORMS OF OPHTHALMIA, VIZ.: THE PURULENT, THE CATARRHAL, AND THE GRANULAR VARIETIES.

By HENRY JULER, F.R.C.S.

Mr. Juler, in commencing his paper, regretted the imperfect knowledge with regard to the specific cause of these complaints, stating that we could, in many instances, only rely upon a diagnosis by the clinical features of the case.

He discussed these diseases under two heads: the acute forms of ophthalmia and the chronic.

In the three common acute varieties, the purulent, the catarrhal, and the granular, many symptoms are similar, for example, the discharge may be purulent in all three kinds. He points out, however, that we are able to distinguish the gonorrheal variety by the presence of the gonococcus of Neisser. The intensity of the symptoms with brawny infiltration of the eyelids, are also more common in this complaint. Acute granular ophthalmia is to be recognized if the "sago-grain" granulations are seen on the palpebral conjunctiva. Their absence, however, is not sufficiently trustworthy to exclude the disease, owing to their existence, in many cases, being masked by the inflammatory infiltration. It usually subsides into a chronic condition. Acute catarrhal ophthalmia, though frequently severe, and oftentimes attended with excessive discharge, conjunctival hemorrhages, and occasioning great discomfort, besides being most contagious, readily subsides under appropriate treatment in about fourteen days. Though the existence of an organism has been proved, more evidence is wanting to show that it is the specific cause of this disease.

Mr. Juler considered that most cases of chronic conjunctivitis came under two headings: the chronic catarrhal and the chronic granular; in other words, a simple and a specific form. He entered into the question of the value of lymphoid follicles or so-called "trachoma bodies," as helping in the diagnosis of these two complaints. He is of opinion that lymphoid follicles existed normally in most cases, having their anatomical site in the cul-de-sac, and that they are, as it were, sentinels which give warning at the approach of foreign or irritating substances, and endeavored to eliminate them if they entered the tissue, consequently were very prone to suffer inflammation, either simple or specific. He believes that only in trachoma are follicles to be found in abnormal situations as on the palpebral conjunctiva or even in the cornea. He showed photomicrographs of these bodies and other histological changes in this

membrane associated with these diseases, one of which illustrated a follicle imbedded in the substantia propria of the cornea in a case in which a progressive pannus had completely disorganized that tissue. So-called "follicular conjunctivitis" he regarded as a simple non-contagious inflammation, allied to chronic catarrhal conjunctivitis, but possessing besides, hyperplastic changes in the normally existing follicles.

He emphasized in conclusion, the difficulty of distinguishing between certain cases and pointed out that their behaviour to treatment is very often the only way of arriving at a diagnosis.

[*This report will be continued in the next issue.—Ed.*]

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If you want a location in a good city, in one of the Southern states (not Tenn.), see page vii of this issue. In this connection, let me ask you to look at all the advertisements in the RECORD, about which special mention will be made in the next issue.

Ophthalmic Week

In the Philadelphia Polyclinic, in November, was devoted to a study of the Ocular Muscles. We have been promised, for publication in the RECORD, a report of the work done in this important branch of eye-study, by the able teachers of ophthalmology in this progressive school for graduates.

The Pink Slip

Will be found in your copy of this issue, if you are due any thing on your subscription account. The time not paid for and the amount due will be stated on the slip. You will greatly favor us by sending your personal check for the amount due, so that the treasury of the RECORD may be full enough to meet current expenses. This issue completes the first half of volume iv.

1894

Will have ended before the appearance of the next number, therefore we let this issue bear to all the readers of the RECORD our best wishes for a merry Christmas and a glad New Year. May we indulge the hope that the friends of the RECORD in this, and in other countries, will give to it the same kind support, literary and financial, in the future, that they have accorded it in the past.

Eight International Ohhthalmological Congress.

This meeting which was held in Edinburgh, August 7-11 last, was not as largely attended as might have been expected, there being only 170 present. The meeting, however, was one composed largely of men who have done, and are doing, good work in the field of ophthalmology, many of them known the world over. The material presented was far too abundant to receive that open and free discussion that would have been profitable; and many papers could not be read for want of time.

About one-fourth of the Congress was composed of American (U. S. A., and Canada) Oculists, many of whom are known wherever ophthalmology is taught. Yet American Ophthalmology was not accorded that place that it richly deserved. On the first day Knapp only was permitted to appear; on the second no paper by an American was presented; on the third day Burnett was accorded a place on the programme; on the fourth day, as if to atone for neglect, or, possibly, that the best might appear last, a large part of the programme was given to Americans in the following order: Theobold, Savage, Risley, Stevens and Gould; but even then the programme was broken by the introduction of one or two who were not down on the card for that day. This complaint (if a true statement can be called a complaint) is not made because the writer did not receive due consideration, for although he came on the last day he had a good audience, and could have occupied his full time had he chosen to do so (in deference to his colleagues to follow him, he ended his remarks in eight minutes). The first hour of every session was devoted to demonstrations. The Americans participating in this feature of the work were: Clark, Risley, Noyes and Ayres.

The papers presented were, as a rule, of a high order of merit, and the discussions were spirited. We have succeeded in securing the services of Dr. A. A. Hubbell, Buffalo, to report the entire proceedings, and that insures that the readers of the Record will have profitable reading. We had thought that this work would devolve on us, but we most gladly transfer the duty to the hands of our able friend, Hubbell. Look for the first part of the report in the next issue.

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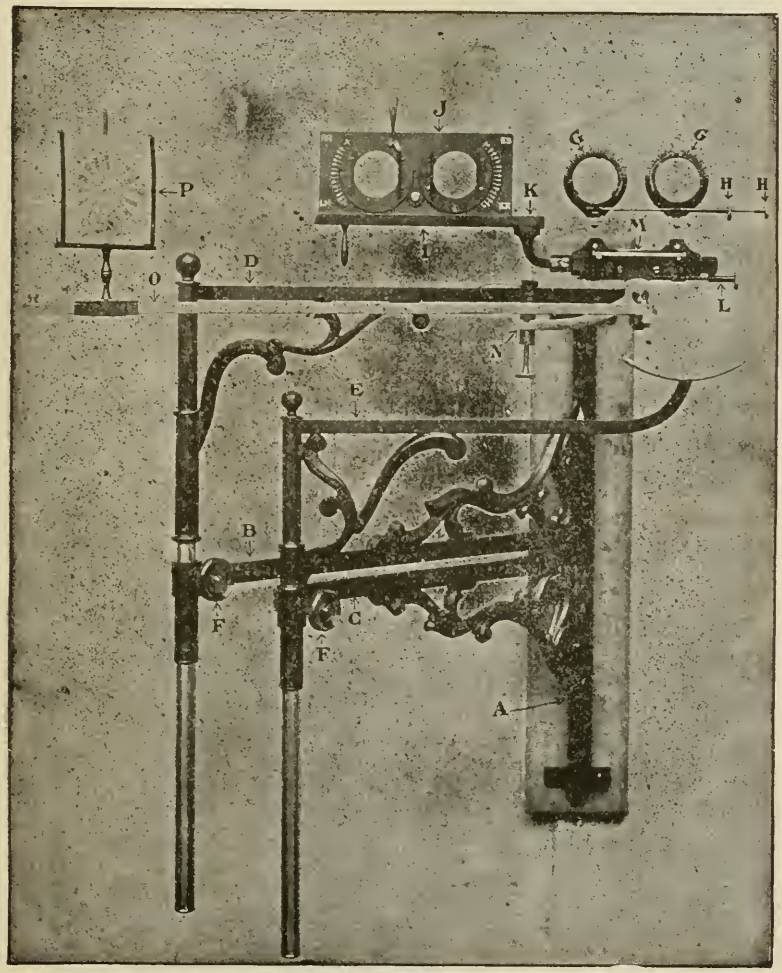

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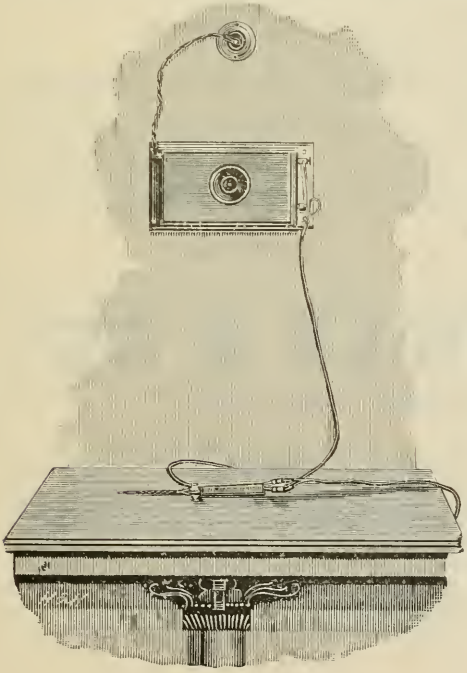

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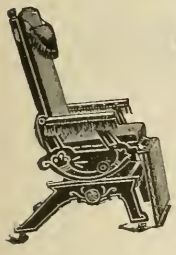

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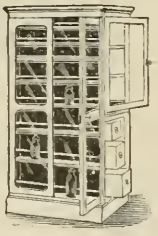

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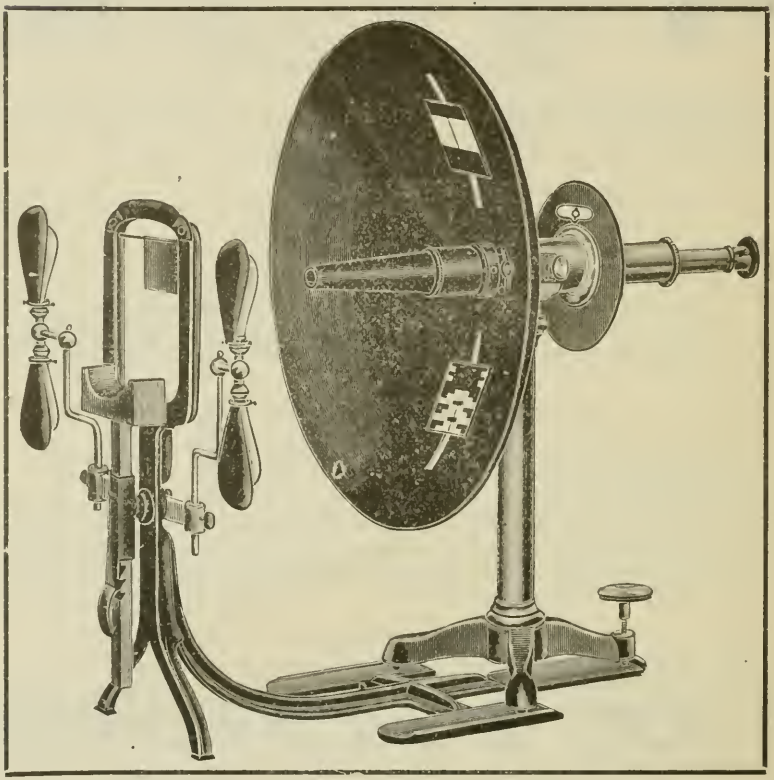

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